

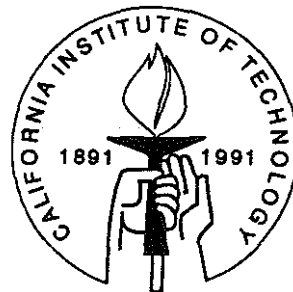
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Experimental Estimates of the Impact of Wage Subsidies

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Abstract

The effects of a wage subsidy program on the duration of insured unemployment are investigated using experimental data. Participation in the experiment was voluntary and about one third of the subjects refused to take the subsidy voucher offered to them. Because subsidies appear to have stigmatic effects which tend to lower participation rates by high-skilled workers, experimental participants have longer average durations of unemployment than non-participants. However, correcting for self-selection, we find that wage subsidies can substantially increase a participant's probability of reemployment. Subsidies are also compared to a search bonus proposal which is also cost effective, but, due to differences in participation patterns, has rather different effects.

Experimental Estimates of the Impact of Wage Subsidies¹

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1 Introduction

Wage subsidies have been proposed as a flexible, efficient and relatively low cost method for reducing unemployment. In a wage subsidy program, job creation and hiring decisions remain the responsibility of private firms, though the cost is partially borne by government. Market incentives should promote the efficient allocation of resources. If workers accumulate human capital in the course of employment, subsidies can be gradually reduced without adverse consequences. In contrast to alternative employment policies, such as job training and public sector employment, which are notoriously expensive and inflexible, subsidy costs are relatively low and can be adjusted to changing labor market conditions.

Though in principle an attractive alternative to conventional unemployment policies, wage subsidies have been tried only rarely in the United States. Theoretical arguments for wage subsidies were made long ago by Pigou (1933) and Kaldor (1936)¹, but recent empirical studies have raised questions about whether wage subsidies are a practical policy for reducing unemployment. Woodbury and Spiegelman (1987) found that offering a wage subsidy for Unemployment Insurance (UI) claimants produced, at best, a modest decrease in the duration of insured unemployment. Burtless (1985), analyzing a more disadvantaged population, reports that subsidy recipients experienced *longer* unemployment spells than non-recipients.

The experimental results are puzzling. Relatively few employers collected the subsidies to which they were entitled, suggesting that many workers did not inform them of

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¹See Kesselman (1969), Burdett (1979), and Layard and Nickell (1980) for more recent analyses.

their eligibility for a wage subsidy. Burtless speculates that being identified as a subsidy recipient has a stigmatic effect on job seekers that outweighs the value of the subsidy. But, if this were the case, why wouldn't workers simply conceal their eligibility? Woodbury and Spiegelman report more positive results (offering a subsidy reduces the total amount of UI benefits paid by more than the cost of the subsidy), but the differences are only statistically significant for white women. They offer no explanation for why the program should be effective for this group and not others.

In this article we reexamine the efficacy of wage subsidies for UI recipients using data from a demonstration project conducted by the Illinois Department of Employment Security (DES). The availability of data from a randomized experiment would appear to make this a simple task. Comparisons of experimental treatment groups with controls permit "model-free" inference. We shall argue, however, that experimental contrasts often are not very powerful for detecting treatment effects and sometimes do not correspond to the differences of primary theoretical interest. Vouchers are shown to be relatively effective for those who use them, though the analysis is complicated by the fact that voucher usage is self-selected.

We also discuss an alternative proposal—a search bonus—which was included in the Illinois UI experiment and which has received favorable notice (Woodbury and Spiegelman, 1987). A careful comparison of the two proposals indicates that the effects of each are quite different, though each appears cost effective. Direct comparisons of experimental effects which ignore differential participation rates give a distorted picture of how the programs work. Both the wage subsidy and bonus programs have a positive impact, but upon different segments of the labor force.

The paper is divided into six sections. Section 2 briefly describes the Illinois UI experiment. Participation rates in the wage subsidy experiment were relatively low and were related to various worker characteristics. In the following section, we consider the general problem of selection bias in policy experiments. Section 4 analyzes the utilization of wage subsidies based upon a simple signalling model. In section 5 the impact of the subsidy and bonus programs on the duration of unemployment and reemployment wages is investigated. The final section presents our conclusions.

2 The Illinois UI Experiment

Our analysis of wage subsidies is based upon data from a demonstration project conducted by the Illinois Department of Employment Security in 1984 and 1985. A total of 17,306 UI claimants in twenty-two DES offices between July and November 1984 were randomly assigned to three experimental groups: a control group (who were granted their normal UI benefits), a wage subsidy group (who were offered a wage subsidy voucher in

addition to their normal benefits), and a search bonus group (who were offered a bonus in addition to their normal benefits). After completing a baseline survey, 5,205 claimants were eliminated because they did not meet criteria for participation in the experiment (described below), leaving a total of 12,101 experimental subjects. In addition, we have eliminated 459 observations because of invalid or missing values, leaving 11,642 complete cases in the analyses reported below.²

The wage subsidy portion of the experiment consisted of a voucher which a worker could present to potential employers as an inducement for his or her hire. If the employer hired the claimant within eleven weeks of the initial UI claim date and retained the claimant for at least four months (for thirty hours or more per week), the employer could submit the voucher for a payment of \$500 from DES. The search bonus portion of the experiment had a similar structure, except that payment was made to the claimant and no employer participation was involved. That is, claimants were not required (and presumably did not) inform employers of their participation in the experiment. They, instead of the employer, submitted the voucher to DES and, in turn, received \$500 if they were employed for at least four months working thirty or more hours per week.

Participation in the experiment was limited to persons between twenty and fifty-four years of age filing initial UI claims who were entitled to a full twenty-six weeks of benefits. Excluded were workers on layoff with a definite recall date, union members who find jobs through a hiring hall, recent veterans, and federal employees. Consequently, the experimental sample is somewhat more homogeneous than the entire UI population, though, because of the exclusion of laid off workers with recall dates, sample members experienced somewhat longer unemployment spells than average.

Subjects were informed of their assignment to treatment groups before they agreed to participate in the experiment. Not everyone who was offered a wage subsidy or bonus agreed to accept the voucher which was offered. As can be seen from Table 1, participation rates were significantly lower in the wage subsidy experiment than in the bonus experiment (68.0% versus 88.8%).

Table 1 about here

There are a number a possible explanations for refusal to participate, ranging from an inability to understand the programs to a possible “stigma” associated with being a subsidy recipient. The wage subsidy, in contrast to the bonus, requires participation both by the claimant and potential employers. For UI claimants to derive any benefit

²In addition to 382 observations for which the claimant’s agreement to participate in the experiment was not recorded, the following data inconsistencies were discovered: 5 claimants did not meet the minimum earnings requirement for UI eligibility, 44 rehire dates preceded initial UI claim dates, 1 notice of hire was submitted without agreement to participate, and 32 vouchers were paid without a notice of hire being submitted. These deletions result in slight differences between the numbers reported here and those found in Woodbury and Spiegelman (1987).

from the wage subsidy, they must persuade an employer that the subsidy can be used to offset their employment cost. For high wage workers, a small subsidy, such as that offered in the experiment, might not be worth the bother.

Burtless (1985) has also argued that subsidies can have a negative impact on recipients' employment prospects by identifying them as members of a low-skill population which has been targeted for government assistance. Consequently, some members of the subsidized population may find it in their interest *not* to inform potential employers of their eligibility for a wage subsidy. This could account for the high refusal rates in the wage subsidy portion of the Illinois experiments. A full-scale implementation of the program could reduce recipient confusion and uncertainty, but stigma effects, if they exist, might well remain.

Both the wage subsidy and search bonus reduced the average duration of unemployment: 40.4% of the claimants in the search bonus experiment and 37.9% of the claimants in the wage subsidy experiment were reemployed within eleven weeks of filing for unemployment insurance, compared to only 35.0% of the controls. The pattern of unemployment spells is displayed in Figure 1 which shows the reemployment rates in each treatment group for the thirty weeks following initial UI claims. Control group members are the slowest to be rehired. By making the vouchers expire after eleven weeks, an incentive was provided to encourage more intensive job search.

Figure 1 about here

The other anticipated effect of the vouchers was to increase the likelihood that claimants would receive an acceptable wage offer in the early period of an unemployment spell and, consequently, experience a briefer duration of unemployment than members of the control group. The wage subsidy, for instance, should increase the amount employers are willing to offer the worker (since the \$500 voucher payment could be used to offset increased labor costs) and, thus, it was anticipated, increase the chances the worker would receive an acceptable wage offer. The bonus, on the other hand, encourages the worker to accept a lower wage than he or she otherwise might, in exchange for the \$500 voucher payment. In both cases, however, the effect would be the same: unemployment spells would be shortened and UI benefit costs would be reduced.

A baseline for comparing post-treatment wages is obtained by examining wage differences prior to the experiment. The left side of Table 2 exhibits the average weekly earnings of claimants in the five full quarters prior to their filing for unemployment compensation (the "base period"). Random assignment of treatments insures that base period earnings are approximately the same in each group (varying between \$254 and \$256 per week). However, agreement to participate in the wage subsidy experiment is clearly related to base period earnings. Nonparticipants earn, on average, \$45 more per week than participants. Note that participation is unrelated to base period earnings in the search bonus experiment.

Table 2 about here

Earnings in the first full quarter after the initial claim (the “post-claim quarter”) are shown on the right side of Table 2. The average earnings in the post-claim quarter are only about 40% of the average earnings in the base period since fewer than half the workers were rehired before the end of the post-claim quarter. There is no significant difference between the average earnings in the control and treatment groups. However, the mean earnings comparison conceals substantial variation within the wage subsidy group. Those who agreed to participate in the wage subsidy experiment had earnings of only \$89 per week in the post-claim quarter, compared to \$94 for controls and \$113 for non-participants assigned to the wage subsidy group. In the search bonus experiment, the pattern was reversed with participants earning \$106 and nonparticipants only \$94.

The two basic factors determining wages in the post-claim quarter are the amount of time employed and, if employed, the wage rate. Since both the wage subsidy and bonus treatments reduce the duration of unemployment, the above earnings differences may reflect amount of time worked rather than differences in reemployment wage rates. Our dataset, however, only contains quarterly earnings, rather than actual wage rates. For workers who were rehired in the post-claim quarter, we have constructed a weekly wage rate by dividing quarterly earnings in the post-claim quarter by the number of weeks worked.³ The average wage calculated this way is subject to possible selection bias since we are conditioning upon the worker having been rehired. This problem is treated in section 7 below.

Table 3 compares the average weekly wages in the base period to those in the post-claim quarter for all workers who were rehired in the post-claim quarter. For control group members, the average weekly wage in the post-claim quarter fell to \$253 from \$264 in the base period. Contrary to expectations, availability of the bonus does not cause unemployed workers to accept lower wages: the decline in average weekly wages from the base period (\$264) to the post-claim period (\$254) is indistinguishable from that found in the control group, and there were no significant differences between participants and non-participants. It appears that the bonus does not cause workers to adjust their reservation wages but to engage in more intensive search to collect the bonus.⁴

Table 3 about here

³The following procedure was used to compute the number of weeks employed in the post-claim quarter. If the rehire date was after the end of the post-claim quarter, the number of weeks employed was zero. If the rehire date was before the beginning of the post-claim quarter, the number of weeks employed was thirteen. In all other cases, the number of weeks employed was equal to the number of days between the rehire date and the end of the post-claim quarter divided by seven and rounded up. This calculation overstates the number of weeks worked since some workers undoubtedly lost their new jobs before the end of the post-claim quarter. Data on refileing for UI were found to be highly unreliable and are not analyzed here.

⁴See Solow (1990) for further discussion.

Participation in the wage subsidy experiment appears to have a perverse effect. Although the average weekly wage in the post-claim quarter in the entire wage subsidy group (\$250) does not differ significantly from those in the other groups, wage subsidy participants fare worse than non-participants or control group members. Those who accepted the wage subsidy voucher had wages of only \$225 per week compared to \$253 for controls and \$301 for those who refused the wage subsidy voucher. These results are similar to those found by Burtless (1985). It appears that participation in the wage subsidy experiment decreases both gross earnings and wage rates. Nonparticipants who are reemployed, however, actually have higher wage rates in the post-claim period than in the base period.

Before concluding that subsidies are harmful, consider how it could be possible for nonparticipants to earn more, on average, than control group members. From an employer's point of view, the two types of job seekers are indistinguishable—neither has a voucher. For one to believe that subsidies are harmful, one would have to simultaneously believe that refusing a subsidy somehow increases workers' wages relative to that of workers who were never offered a subsidy (the control group). This is entirely implausible.

The only possible explanation for the post-claim wage differences within the wage subsidy group is self-selection of experimental participants. In the base period, as noted previously, those who refused the wage subsidy voucher had higher weekly wages than those who accepted the voucher. This means that comparisons of participants and non-participants that fail to control for differences between the groups will not accurately reflect the impact of wage subsidies. On the other hand, grouping together those who accepted and refused the wage subsidy voucher results in a comparison that mixes the wage subsidy effect with the participation effect. The next section discusses how these effects might be disentangled.

3 Bias in Social Experiments

Wage subsidies have both supply and demand effects which are difficult to sort out without making strong assumptions about how workers and employers would respond to the availability of a subsidy. The primary advantage of an experimental trial is that behavioral responses to a subsidy are directly observable. Woodbury and Spiegelman (1987, p. 518) argue that random allocation of treatments obviates the need for any econometric modeling:

It follows that comparisons between the Claimant Experiment group and the control group (or between the Employer Experiment group and the control group)

implicitly control for all observed and unobserved variables that may have contributed to the outcomes that are of interest—duration of insured unemployment and post-reemployment earnings. Thus, a simple comparison of the mean weeks of insured unemployment for members of either experimental group with mean weeks of unemployment for members of the control group will show the impact of the treatment in question on the duration of insured unemployment.

While randomization is very useful, it does not cure all of the problems in estimating the impact of a wage subsidy.

Randomized experiments are now used with some regularity to evaluate alternative policy proposals. In observational studies, self-selection of “treatments” is always a potential source of bias. Random allocation of treatments guarantees that members of the treatment group differ in no systematic way from the control group (if sample size is sufficiently large). There is more, however, to experimental design than random allocation of treatments. The design of the UI experiment is different from that of the typical biomedical experiment and these differences have implications for how data from the experiment should be analyzed.

The differences between social and biomedical experiments are evident if one compares the UI experiment to the 1954 field trial of the Salk poliomyelitis vaccine.⁵ The polio experiment involved 750,000 children from grades one to three. Nearly half the parents did not give permission to have their children inoculated. Participating children were randomly assigned to treatment and control groups. The experiment was run double-blind with controls receiving a placebo so that neither they nor the experimenters would know whether they had received the vaccine or not. Important differences between the designs of the polio and UI experiments are (1) the order of agreement to participate and assignment of treatments, (2) use of a placebo, and (3) observability of treatment.

Both the polio and UI experiments had high refusal rates. In fact, the proportion of parents who refused to have their children inoculated with the polio vaccine was higher than the proportion of UI claimants who refused the wage subsidy voucher. Nonparticipants also differed systematically from participants in the polio trials. Children vary enormously in their susceptibility to polio. Infants who are exposed to the polio virus develop a natural immunity that protects them when they are older. Exposure to the virus as an infant is related to family hygiene so the risk of contracting polio as a child is negatively correlated with family background variables such as parent’s education and income. Since parents with lower education and income were less likely to permit their children to be inoculated, nonparticipants were less vulnerable to polio.

In the polio experiment, as in most scientific experiments, agreement to participate is secured before treatments are allocated. Eliminating children whose parents would

⁵We are relying upon Francis *et al.* (1955). See also Brownlee (1954) and Meier (1972).

not allow them to be inoculated gives a biased subsample of the original experimental population. This bias was substantial: the incidence of polio was 71 confirmed cases per 100,000 children who agreed to participate and received the placebo compared to 46 cases per 100,000 nonparticipants. However, random assignment of treatments *after* agreement to participate ensures that the treatment and control groups are random subsamples of the *participating* populations.

The polio experiment also suffers from attrition after treatments have been assigned. Some children receive only one or two injections of the vaccine or placebo. The level of post-assignment attrition was modest (about 4% of those who initially agreed to participate). More significantly, since subjects did not know whether they were to be injected with the vaccine or a harmless saline solution, treatment and control group members are equally likely to drop out of the experiment. Thus, the use of a placebo ensures that post-assignment attrition is independent of which treatment was assigned. Deleting those who did not receive a full set of injections from both the control and treatment groups further restricts the sample without making the initial randomization ineffective.

In contrast, all refusals in the UI experiment occurred after treatments were assigned. Furthermore, nonparticipation in the UI experiment is strongly correlated with treatment since there are no refusals to participate in the control group. (In fact, controls were never informed that they were participating in an experiment.) Eliminating refusals from the sample would make the treatment group self-selected and bias comparisons with the control group (which remains a random sample of the experimental population).

To complicate matters further, participation in the UI experiment is only partially observable. Some subjects may have taken the wage subsidy voucher without making any subsequent use of it. It is as if the experimenter did not administer the polio vaccine directly to the subjects, but distributed the vaccine and asked the subjects to administer it to themselves at home instead. If some of the subjects in the treatment group disobeyed instructions and did not take the vaccine, they would actually belong to the control group though the experimenter would misclassify them as treatment group members.

The two different experimental designs are summarized in Table 4. In the polio experiment, initial refusals are not assigned to either the treatment or control group. The relatively few post-assignment dropouts can be grouped with the pre-assignment refusals and the analysis limited to those actually participating in the experiment. In the UI experiment, we cannot distinguish between control group members who would have utilized a wage subsidy voucher and those who would not have. The treatment group can be divided into those who accepted the voucher and those who refused it, though, as noted above, some of those in the treatment group who accepted the voucher may not have used it to obtain employment.

Table 4 about here

There are two possible methods for analyzing experiments with nonparticipation. The method normally used is to eliminate nonparticipants (both pre and post-assignment refusals) and to compare the mean response of control and treatment group members who agreed to participate. We call this the “complete cases” comparison. The alternative is to compare *all* control group members with *all* treatment group members, regardless of whether they agreed to participate in the experiment. We call this the “full sample” comparison.

Let Y denote the response variable of interest (e.g., rate of polio or duration of insured unemployment). Let A be a dummy variable indicating whether the subject agreed to participate in the experiment ($A = 1$ if agreed, $A = 0$ if refused) and T be an indicator of treatment assignment ($T = 1$ if treatment, $T = 0$ if control). In the polio experiment, agreement should be interpreted as willingness to be inoculated. Similarly, agreement to participate in the UI experiment should be interpreted as willingness to accept the wage subsidy voucher if it were offered. If treatments are assigned only after agreement to participate, as in the polio experiment, T is not observed if $A = 0$. If subjects are informed of their treatment assignments before they agree to participate, as in the UI experiment, then A is not observed if $T = 0$.

In both designs, A and T are independent. In the polio experiment no treatment assignment is made if the subject initially refuses to participate, so there is no loss of generality in assuming $P(T = 1|A = 0) = P(T = 1|A = 1)$. (Because of the placebo, post-treatment refusals are also independent of treatment assignment.) In the UI experiment, treatments are assigned randomly to the entire experimental population. This means that if those assigned to the control group were offered a wage subsidy voucher, the expected proportion of refusals would be the same as that in the actual treatment group. Therefore, let $\pi = P(A = 1) = P(A = 1|T)$.

There is some ambiguity about the definition of treatment effect. What is usually meant by “treatment effect” is, in fact, an average of treatment effects across some population. The complete cases method estimates the average treatment effect for those who agreed to participate,

$$\delta = E(Y|T = 1, A = 1) - E(Y|T = 0, A = 1).$$

The full sample method estimates a different average,

$$\bar{\delta} = E(Y|T = 1) - E(Y|T = 0).$$

Which is the relevant comparison? $\bar{\delta}$ is a weighted average of the treatment effect for those who agree to participate and those who refuse,

$$\bar{\delta} = \pi\delta + (1 - \pi)\delta',$$

where

$$\delta' = E(Y|T = 1, A = 0) - E(Y|T = 0, A = 0)$$

is the average treatment effect for the refusals. However, treatment is irrelevant for refusals. Someone who refuses to be inoculated with the vaccine faces the same risk of polio as if he had been given a placebo. Likewise, someone who refuses a wage subsidy voucher is in the same position as if he had never been offered a voucher. Thus, $\delta' = 0$ and, as a consequence, $\delta = \bar{\delta}/\pi$.

It is common practice in social experiments to call δ the “average treatment effect,” though it is, more precisely, the average effect of being *offered* the treatment. Since the effect of being offered and refusing the treatment is known *a priori*, the interesting quantity is the effect of the treatment upon those who agree to participate, namely δ . Though the full sample comparison gives a biased estimate of δ , it is still feasible to test hypotheses about δ using the full sample comparison. For example, the hypothesis $H_0 : \delta = 0$ is equivalent to $H'_0 : \bar{\delta} = 0$ if $\pi > 0$. The latter hypothesis can be tested using the full sample comparison, though the test may not be very powerful. Since $\bar{\delta}$ can be small because either δ is small or π is small, a fair evaluation requires that we distinguish these two cases.

It is constructive to consider what conclusions would have been drawn from the polio experiment if it had been conducted in the same way as the UI experiment. If the treatments had been assigned in the polio experiment before agreement to participate had been secured, half of the initial refusals would have been assigned to the control and treatment groups. Apportioning the cases of polio among the refusals to the treatment groups in equal proportions yields a polio rate of 59 per 100,000 for the controls versus 36 per 100,000 in the treatment group. Although this difference using the full sample comparison is still statistically significant ($z = -4.48$, $p < 0.01$), the estimated treatment effect is only half as large as that estimated using the complete cases comparison (71 cases per 100,000 among the controls versus 28 per 100,000 in the treatment group; $z = -6.01$, $p < 0.01$). The evidence from this comparison is weak enough that population wide vaccinations might have been delayed or avoided altogether.

There are a number of lessons in the polio experiment for analysis of the UI experiment. First, a small average effect in a heterogeneous population may mask substantial effects among subgroups in the population. Second, most social experiments have voluntary participation *after* subjects have been assigned to treatment groups. We know, however, on *a priori* grounds that there is no treatment effect for nonparticipants. If participation rates are low, it may be hard to detect a substantial treatment effect among the portion of the population who would voluntarily participate using the full sample comparison. Third, response differences between nonparticipants in the treatment group and the control group must, of necessity, represent the effects of self-selection.

The complete cases comparison is not possible in the UI experiment because the controls do not receive a placebo and have no opportunity to refuse to participate. The design of the unemployment experiment does, however, allow us to compare participants

and nonparticipants in the treatment group:

$$\delta'' = E(Y|T = 1, A = 1) - E(Y|T = 1, A = 0).$$

Since the decision to participate is made by the subjects, this comparison is subject to selection bias. We can, however, correct for selection bias using

$$\delta = \delta'' + \frac{1}{\pi} [E(Y|T = 1, A = 0) - E(Y|T = 0)]. \quad (1)$$

The second term of (1) is a form of selectivity correction as it compares the response of those refusing the treatment to those who were never offered it. If the latter difference is zero, the refusals can be treated as a random sample of the experimental population.

Table 5 reproduces the full sample comparisons of Table 1 along with estimates of the treatment effects for those who accepted the voucher offered to them. The full sample estimate of the treatment effect is $\bar{Y}_1 - \bar{Y}_\bullet$, where \bar{Y}_1 is the mean of the response variable Y among all those assigned to the treatment group and \bar{Y}_\bullet is the mean of Y among all controls. The “adjusted comparison” in the second column of Table 5 is $(\bar{Y}_1 - \bar{Y}_\bullet)/\hat{\pi}$, where $\hat{\pi}$ is the proportion of treatment group members who agree to participate.⁶ The estimated wage subsidy effects for those accepting the voucher are, of course, much larger than the full sample effects. The wage subsidy effects are still somewhat smaller than the search bonus effects, but the adjusted effects are of similar magnitude. (The adjustments are conservative since actual voucher usage is overstated by agreement to participate.) The apparent advantage of the search bonus in the full sample comparisons is mostly attributable to its higher participation rate.

Table 5 about here

There are still some problems with the comparisons in Table 5. First, reemployment wages are not observed for those who are not rehired.⁷ Second, although these comparisons are not confounded with other variables, introducing additional controls will generally improve the power of hypothesis tests. The method of adjusting for participation rates in equation (1) is easily modified to accommodate covariates, since

$$E(Y|X, T = 1, A = 1) - E(Y|X, T = 0, A = 1) = E(Y|X, T = 1, A = 1) - E(Y|X, T = 1, A = 0) + \frac{1}{\pi(X)} [E(Y|X, T = 1, A = 0) - E(Y|X, T = 0)], \quad (2)$$

assuming $\pi(X) = P(A = 1|X) = P(A = 1|X, T)$ and $E(Y|X, T = 1, A = 0) = E(Y|X, T = 0, A = 0)$ as before. In the next section, we estimate a logit model for the

⁶An equivalent estimate of δ (from Equation 1) is $\bar{Y}_{11} - \bar{Y}_{10} + (\bar{Y}_{1\bullet} - \bar{Y}_0)/\hat{\pi}$, where \bar{Y}_{11} and $\bar{Y}_{1\bullet}$ denote the mean responses for those assigned to the treatment group who agreed and refused to accept the voucher, respectively.

⁷This is also a problem in Woodbury and Spiegelman (1987), who by analyzing mean earnings of subjects only with positive earnings, vitiate the randomization that they rely upon elsewhere.

participation probability $\pi(X)$. The following section estimates models for $E(Y|X, T, A)$ where Y is either duration of unemployment or reemployment wage. All of these models have the character of “reduced form” equations. Agreement to participate is clearly endogenous and would require a more elaborate treatment in a structural model. Here it suffices to fit the data reasonably well.⁸

4 Utilization of Wage Subsidies

A simple signalling model, along the lines of Spence (1973), clarifies the informational content of wage subsidies in a competitive labor market. We envision a situation in which there are many potential employers for a worker, but employers cannot directly observe the worker’s productivity at the time of a hiring decision. We assume that employers share a common assessment m_s of the average marginal revenue product for workers eligible for subsidies and m_u for workers ineligible for subsidies. Let p denote the fraction of workers eligible for subsidies and s the amount of the subsidy. If the worker identifies himself as being a member of the subsidy population, the employer would be willing to offer a wage of

$$w_s = m_s + s,$$

assuming employers are risk neutral. Suppose, however, that only a fraction π of subsidized workers identify themselves as being eligible for the wage subsidy. Then, the average marginal revenue product of job applicants without wage subsidies is

$$w_u(\pi) = \frac{(1-p)m_u + p(1-\pi)m_s}{1-p\pi}.$$

In a competitive labor market, this is the amount that an employer would be willing to offer an unsubsidized worker if π were known.

Since the subsidized worker decides whether or not to inform the employer of his or her eligibility for the wage subsidy before the employer makes a wage offer, we treat the worker as a Stackelberg leader. If $s \geq m_u - m_s$, all subsidized workers will inform employers of their eligibility for the wage subsidy since $w_s > w_u(\pi)$ for all $0 \leq \pi < 1$. If $s \leq (1-p)(m_u - m_s)$, none of the subsidized workers inform employers of their eligibility since $w_s < w_u(\pi)$ for all $0 < \pi \leq 1$. In the intermediate case where $(1-p)(m_u - m_s) < s < m_u - m_s$, the subsidized workers play a mixed strategy giving the equilibrium utilization rate

$$\pi^* = \frac{s - (1-p)(m_u - m_s)}{ps}.$$

⁸We could also estimate models for $E(Y|X, T)$ and estimate the response effect for participants using $[E(Y|X, T=1) - E(Y|X, T=0)]/\pi(X)$. The method used in the text gives similar results and has the advantage of emphasizing the nature of nonparticipation in the UI experiment.

For subsidies in this range, the utilization rate π^* is increasing in p and decreasing in $(m_u - m_s)/s$. From this analysis, a few simple conclusions can be drawn. First, if the difference in productivity levels between the unsubsidized and subsidized populations is large relative to the amount of the subsidy, the utilization rate will be lower. Second, if the proportion of the labor force eligible to receive wage subsidies is large, the utilization rate will be higher.

It might be argued that UI recipients do not have lower productivity levels than non-UI recipients, so there should be no stigma attached to being identified as eligible for a UI-based subsidy. This argument is incorrect, because it confuses average and marginal differences between subsidy recipients and non-recipients. In the above analysis, m_u and m_s should be interpreted as the expected marginal revenue product of subsidized and unsubsidized workers *conditional upon all observable characteristics*. The relevant comparison is not between *all* subsidy recipients and non-recipients, but between two workers who would appear similar to employers, except that one qualifies for a UI-based employment subsidy and the other does not. Similarly, p is the proportion of all workers with a given set of observable characteristics who qualify for a subsidy.

The wage subsidies in the Illinois experiment, unlike those in the Phoenix experiment analyzed by Burtless (1985), were not targeted toward a particularly disadvantaged population. The only information recipients reveal by identifying themselves as eligible for the subsidy is that they qualified for unemployment insurance. Since the design of the experiment was not publicized, it is possible that employers drew unwarranted inferences about why recipients qualified for the subsidy. For relatively low-skilled workers, there is unlikely to be much additional information contained in the fact that the worker is eligible for the wage subsidy, so the ratio $(m_u - m_s)/s$ would be small, leading to high utilization rates. For high-skill workers, the potential stigma effects from carrying the subsidy are more serious. When reducing output, employers tend to layoff workers with low productivity relative to their wages, resulting in a population of UI recipients who have lower productivity than their observable characteristics would suggest. In this case, the value of $(m_u - m_s)/s$ is large and the implied utilization rate is low. We show that this expectation is borne out by the data.

Actual utilization rates are unknown since some workers who agree to participate in the program may never inform potential employers of their eligibility for a wage subsidy. However, non-participants are not supplied with vouchers so agreement to participate is a necessary but not a sufficient condition for utilization of the voucher. We use agreement to participate as a proxy for utilization with the recognition that it is upwardly biased. Many of the subjects who accepted the subsidy voucher (and whom we classify as participating) did not make use of the voucher in seeking employment. Spiegelman and Woodbury (1987, ch. 7) report the results of a follow-up survey in which 34% of those who agreed to participate either had forgotten about the voucher or admitted not using it. Of the follow-up sample, only 29% claimed to have used the voucher. But even this

measure of participation is certainly too high, since there would be a natural tendency for respondents to report voucher usage to those conducting the experiment.

We do not have access to the follow-up data, but, in any event, there is no reason to prefer the survey reports of usage to the agreement to participate measure. In fact, our use of the participation proxy need not be a source of bias. We might view propensity to participate as a continuous variable divided into three ordered categories: refused the voucher (corresponding to the lowest propensity to participate), accepted the voucher without using it (corresponding to intermediate values), and active usage (corresponding to the highest values). With this structure, the logit estimates reported below will still be consistent (except for the intercept, which is not identified).

The preceding theoretical analysis indicates that the probability of utilization is a function of the ratio of the subsidy to the difference in productivity between recipients and nonrecipients perceived by employers. Since the subsidy amount does not vary in this experiment, it suffices to relate participation rates to characteristics of recipients which might be associated with their productivity levels.⁹ Table 6 presents logit estimates for the probability of agreeing to participate in each of the experiments as a function of the claimant's characteristics (sex, race, age, base period earnings, and UI benefit level¹⁰).

Table 6 about here

Participation rates are generally about 21 percent higher in the bonus experiment than in the subsidy experiment, as indicated by the larger value of the constant term in that equation. Hispanics were less likely to participate in either the subsidy or the bonus experiment. A study of earnings, benefits, and the remaining demographic indicators revealed that Hispanics were about 7 percent less likely to participate in the bonus experiment and 22 percent less likely to participate in the subsidy experiment (perhaps because of language problems). Differences in participation rates between other demographic groups are small. Blacks were a bit less likely than whites to participate in the bonus experiment, but equally likely to participate in the subsidy experiment. Males were significantly more likely to participate in either experiment (by about 6 percent in the subsidy experiment and 3 percent in the bonus experiment).

There is no significant impact of either base wage level (average weekly earnings in the two full quarters prior to initial filing of a UI claim) or weekly UI benefit level on the search bonus experiment. In the wage subsidy experiment, on the other hand, every additional \$100 of weekly income in the base period reduces participation by about 2 percent. This fact provides some support for the stigma explanation advanced above. High wage workers appear to be reluctant to utilize the subsidy. Base period earnings,

⁹It would be desirable for several different levels of subsidy to be used in future experiments.

¹⁰During the period of the experiment, regular UI benefits were 48% of base period wages, with a ceiling of \$154 per week.

however, have a much smaller effect on participation than do weekly UI benefit levels. The average UI benefit in our sample is \$119 with a standard deviation of about \$40. A \$40 increase in weekly UI benefits causes nearly a 3 percent decline in participation in the subsidy experiment. Contrast this effect to that observed in the search bonus experiment where the coefficient is positive (though not significant). Wage subsidies are much more attractive to those with lower benefits. What little self-selection there is in the bonus program does not screen out high earnings workers to any significant degree.

5 Effects on Employment and Earnings

In each period a worker is classified as either employed or unemployed. The basic idea is to specify a hazard function giving the probability that an unemployed individual becomes employed conditional on his or her employment history and demographic characteristics. Let S_{it} denote the employment status of worker i in period t , where $S_{it} = 1$ if employed and $S_{it} = 0$ otherwise. The hazard at period t for a subject assigned to treatment $T_i = j$ is the probability of finding employment in period t conditional upon being unemployed in periods $1, \dots, t-1$. We adopt a logistic specification for the hazard function,

$$P_{it} = P(S_{it} = 1 | S_{i1} = \dots = S_{i,t-1} = 0, X_{it}, T_i = j) = \frac{\exp(\beta'_j X_{it})}{1 + \exp(\beta'_j X_{it})},$$

where X_{it} is a set of time-varying covariates. The probability that an individual unemployed in period t fails to find a job in that period is $1 - P_{it}$.

Each worker in the experiment becomes unemployed and files for UI benefits during either the second or third quarter of 1984 and we follow their employment status for 26 weeks after their initial claim date. Let Y_i denote the number of weeks the i th subject is observed to be unemployed, up to a maximum of 26 weeks. Any spell of unemployment longer than 26 weeks is censored. It follows that the conditional probability of observing an unemployment spell of length Y_i which is either complete ($c_i = 0$) or censored ($c_i = 1$) is:

$$L_i(Y_i, c_i) = P_{i,Y_i}^{1-c_i} (1 - P_{i,Y_i})^{c_i} \prod_{t=1}^{Y_i-1} (1 - P_{it}) \quad (3)$$

From (3) we form the log likelihood for the full sample which can be optimized with a conventional multinomial logit program. We have estimated weekly, monthly, and quarterly models with roughly similar results, but will only report results using a weekly periodicity here.

Estimates of the effects of treatment (assignment to either the wage subsidy or bonus groups) and agreement to participate (*i.e.*, did the worker accept the voucher) are shown in Table 7. Controls for age, race, sex, prior wages, and UI benefit level are included in

each specification. Separate effects of experimental treatment, agreement to participate, and UI benefit level are estimated for the first and second quarters of each UI spell. Finally, we include either a linear time trend (number of weeks unemployed) or a set of period dummies to capture duration dependence not explained by other variables. The first three columns of Table 7 contain models estimated for each of the three experimental groups. Since the demographic variables have similar effects in each group, data from the three groups were pooled to obtain the estimates in the last two columns of Table 7. To simplify the discussion, we interpret the coefficients in terms of percentage changes in hazard rates (holding the remaining variables at their mean values) and focus upon the pooled estimates in the fourth column of the table.

Table 7 about here

Once again, we find important differences between the wage subsidy and search bonus programs. The weekly escape rate in the control group is about 3.2%. There is no significant effect of assignment to the search bonus group for those who refuse to accept the voucher. Participation in the bonus experiment increases the probability of reemployment in any week during the first quarter of an unemployment spell by about 0.6%. In contrast, there is no significant effect of participation in the wage subsidy experiment beyond assignment to the treatment group. Subjects who refuse the wage subsidy voucher experience a 0.4% increase in their weekly probability of reemployment in the first quarter of an unemployment spell. Since the program incentives disappear after approximately three months, it is not surprising that there are no significant effects of either treatment or agreement to participate during the second quarter of the spell.

Controlling for a limited set of individual characteristics, we see that both participants and nonparticipants in the wage subsidy experiment become employed significantly faster than those in the control group. If it were possible to match controls and nonparticipants in the wage subsidy experiment on all relevant attributes, average duration of unemployment should be the same for comparable individuals. However, the estimates in Table 7 imply shorter unemployment spells for those who refused the subsidy than for controls—not because of any treatment effect (since they clearly derived no benefit from the subsidy), but because of self-selection.

We do not have enough measures of individual characteristics to adequately control for the effects of self-selection so the estimates in Table 7 cannot be given a structural interpretation. Instead, we use equation (2) to estimate the impact of either wage subsidy or bonus participation on workers of different types. For example, the expected duration of unemployment for a 40 year old black female with base period wages of \$250 who accepts the wage subsidy voucher is 20.8 weeks, a reduction of 0.72 weeks from her expected duration if she had not been offered the voucher. A similar calculation indicates that a reduction of 0.93 weeks is attributable to the bonus. For those who agree to participate, the wage subsidy and search bonus have similar size effects on employment, though, of course, the average effect of the wage subsidy is considerably smaller because

of lower participation rates.

The other response variable of interest is the level of reemployment wages. One would expect the shorter durations of unemployment found in the experimental groups to have resulted from shifts in labor supply or demand and to be reflected in reemployment wage rates. We estimated a regression equation relating weekly wages in the post-claim quarter to experimental treatment, agreement to participate, and base period earnings. Ordinary least squares (OLS) estimates are displayed in the first column of Table 8.¹¹

Table 8 about here

The average weekly wage equations suffer from potential selection bias since the sample is restricted to those workers who gain employment in the quarter following their initial UI claim. To correct for censoring, we use the selectivity correction developed in Dubin and McFadden (1984). In our case, the selection term takes the form $-\frac{[\hat{P}_{i,Y_i} \log \hat{P}_{i,Y_i} + (1 - \hat{P}_{i,Y_i}) \log(1 - \hat{P}_{i,Y_i})]}{\hat{P}_{i,Y_i}}$, where \hat{P}_{i,Y_i} is the estimated probability of escape for the period in which the worker is rehired. The escape probabilities were estimated using the duration model reported in the fourth column of Table 7. The selectivity-corrected estimates are presented in the last column of Table 8.

The results in Table 8 are a reprise of our earlier findings. Nonparticipants in the wage subsidy experiment earn \$43 more per week than their counterparts in the control group, who earn about the same amount as participants in the subsidy experiment. There is no discernible effect of the bonus upon nonparticipants. The wages of search bonus participants are estimated to be \$31 higher than controls, though this difference is imprecisely estimated. (A 95% confidence interval ranges from -\$6 to \$56.)¹²

It is not clear on *a priori* grounds whether the benefits of a wage subsidy accrue to workers or their employers. If workers maintain a constant reservation wage, the subsidy should induce employers to increase employment without any change in wage levels. On the other hand, the subsidy may cause workers to adjust their wage demands upward. In the first case, employers are better off because the subsidy reduces the effective cost of labor, while in the latter workers capture the subsidy. The most probable outcome is an increase in both employment and wage rates.

A search bonus has the same real effects as a wage subsidy if nominal wage rates are adjusted to incorporate the present value of the bonus. In nominal terms, however, we would expect higher employment levels to be accompanied by lower wages. A simulation

¹¹The estimated coefficient on lagged wages is similar to that found by Addison and Portugal (1989). They include duration of unemployment in the reemployment wage equation instead of a selectivity correction.

¹²The estimated coefficient of the selectivity correction term implies a correlation of 0.41 between the unobservables in the average weekly wage equation and the latent logistic error in the duration model for the period of escape (Dubin and McFadden, 1984: p. 352).

of wage effects using equation (2) confirms that the wage subsidy raises participant wages slightly (by \$23), but, contrary to expectations, the bonus also raises wages (and by a larger amount, \$30). One possible explanation is that bonuses causes workers to increase the intensity of job search without any change in their reservation wage level.

6 Discussion and Conclusions

Experiments are very helpful in evaluating policy alternatives, but their analysis is sometimes more problematic than it first appears. The design of most social experiments differs substantially from what is typical in biomedical applications. Random allocation of treatments does not determine what the subjects do with the treatments. The standard experimental comparisons actually measure the effect of being offered the treatment rather than using it. Since the effect of the treatment is known to be zero for refusals, it may be difficult to detect substantial effects for participants when the participation rate is low. In principle these problems could be reduced by securing agreement to participate before assignment of treatments, but there is no way to control post-assignment attrition without a placebo.

Analyses that ignore participation rates tend to confuse low participation with small treatment effects on participants. We have demonstrated that wage subsidies and search bonuses have similar size effects upon participants, but have substantially different patterns of participation. Lower participation rates imply that the potential total benefits from the subsidy program are smaller than those from the search bonus program, but benefits constitute only one side of the cost-benefit ledger. Though Woodbury and Spiegelman (1987) found that the benefit-cost ratio was higher for wage subsidies than search bonuses, their results have been interpreted as showing that wage subsidies are ineffectual (Kerachsky and Corson, 1988: p. 115).

Wage subsidies and search bonuses have rather different effects on different segments of the labor market. Bonuses appear to work primarily by increasing the intensity of search, rather than by modifying reservation wage levels or employer demand for labor. Bonus vouchers are accepted by nearly all workers and are paid largely to those who would find employment in the normal course of events, albeit after collecting less unemployment compensation than they currently do. Wage subsidies, on the other hand, will typically be refused by high wage workers who appear reluctant, for whatever reason, to identify themselves as beneficiaries of a government assistance program. For low wage workers, particularly those near the minimum wage level, a wage subsidy has distinct advantages over a search bonus. More generally, wage subsidies tend to be cost effective because they are paid only to those unemployed workers who use them as incentives for employers to hire them. If the subsidy does not assist the worker in finding a job, the worker won't utilize the subsidy and it won't be paid.

Table 1: Summary of Illinois UI Experiments

	Control	Wage Subsidy	Search Bonus
Initial Claimants	3931	3781	3930
Agreed to Participate	–	68.0%	88.8%
Rehired Within 11 Weeks	35.0%	37.9%	40.4%
Notice of Hire Submitted	–	5.0%	18.4%
Voucher Paid	–	2.8%	13.5%
UI Benefits Paid (First Spell)	\$2278	\$2166	\$2085
UI Benefits Paid (Benefit Year)	\$2492	\$2422	\$2331

Table 2: Average Gross Weekly Earnings

	<i>Base Period</i>			<i>Post-Claim Quarter</i>		
	Agreed to Participate	Refused to Participate	All Claimants	Agreed to Participate	Refused to Participate	All Claimants
Control	–	–	254	–	–	94
Wage Subsidy	240	285	254	89	113	97
Search Bonus	256	252	256	106	94	105
<i>n</i>	5953	1611	11410	5953	1611	11410
<i>F</i>	12.0	8.5	0.1	13.0	3.6	4.1
<i>p</i>	<0.01	<0.01	0.91	<0.01	0.06	0.02

Table 3: Average Weekly Wage Rates*

	<i>Base Period</i>			<i>Post-Claim Quarter</i>		
	Agreed to Participate	Refused to Participate	All Claimants	Agreed to Participate	Refused to Participate	All Claimants
Control	—	—	264	—	—	253
Wage Subsidy	247	289	261	225	301	250
Search Bonus	265	259	264	256	242	254
<i>n</i>	2894	743	5308	2894	743	5308
<i>F</i>	6.6	3.2	0.3	5.1	1.9	0.1
<i>p</i>	<0.01	0.07	0.77	0.02	0.17	0.93

*Includes only workers rehired before end of post-claim quarter.

Table 4: Experimental Design

<i>Polio Experiment</i>			
		Assignment	
		Control	Treatment
Participation	Agreed	Control	Treatment
	Refused	Refusals	

<i>Unemployment Experiment</i>			
		Assignment	
		Control	Treatment
Participation	Agreed	Control	Treatment
	Refused		Refusals

Table 5: Adjusted Estimates of Treatment Differences*

Response Variable	<i>Wage Subsidy</i>		<i>Search Bonus</i>	
	Unadjusted	Adjusted	Unadjusted	Adjusted
Weeks of Insured	-0.69	-1.01	-1.34	-1.51
Unemployment	(-2.35)	(-2.35)	(-5.36)	(-5.35)
UI Benefits Paid	-113	-166	-193	-218
(First spell)	(-4.07)	(-4.07)	(-5.69)	(-5.68)
UI Benefits Paid	-70	-102	-161	-182
(Benefit year)	(-2.09)	(-2.09)	(-4.85)	(-4.84)

**t*-statistics in parentheses.

Table 6: Logit Estimates of Participation Rates*

	Wage Subsidy	Search Bonus
Constant	1.802 (3.661)	2.227 (3.156)
Male	0.289 (3.872)	0.299 (2.823)
Black	0.002 (0.024)	-0.294 (-2.479)
Hispanic	-0.944 (-7.257)	-0.655 (-3.763)
Log Age	-0.138 (-0.963)	-0.018 (-0.088)
Base Weekly Wage (in \$100's)	-0.107 (-3.643)	-0.003 (-0.066)
Weekly Benefit Amount (in \$100's)	-0.299 (-2.257)	-0.093 (-0.491)
Log Likelihood	-2257	-1342
<i>n</i>	3701	3865

**t*-statistics in parentheses.

Table 7: Logistic Hazard Models for Unemployment Spells*

	Control	Wage Subsidy	Search Bonus	Pooled	Pooled
Constant	-1.72 (-5.30)	-1.24 (-3.87)	-1.79 (-5.61)	-1.59 (-8.68)	Period dummies
Weeks Unemployed	-0.02 (-2.86)	-0.03 (-4.99)	-0.02 (-3.36)	-0.02 (-6.62)	
Log Age	-0.40 (-4.27)	-0.46 (-5.03)	-0.41 (-4.51)	-0.42 (-7.96)	
Black	-0.63 (-10.64)	-0.58 (-9.82)	-0.60 (-10.39)	-0.60 (-17.80)	-0.61 (-17.80)
Hispanic	-0.26 (-2.92)	-0.30 (-2.95)	-0.28 (-3.13)	-0.27 (-5.17)	-0.27 (-5.17)
Male	0.06 (1.35)	-0.07 (-1.56)	0.06 (1.22)	0.02 (0.57)	0.02 (0.56)
Base Weekly Wages (in \$100s)	0.06 (3.72)	0.06 (3.14)	0.04 (2.43)	0.05 (5.30)	0.05 (5.34)
Weekly Benefit (in \$100s)					
First Quarter	-0.20 (-2.40)	-0.23 (-2.79)	-0.10 (-1.23)	-0.17 (-3.60)	-0.03 (-5.22)
Second Quarter	-0.29 (-3.18)	-0.16 (-1.64)	0.02 (0.20)	-0.15 (-2.72)	0.06 (0.90)
Wage Subsidy Treatment					
First Quarter	—	—	—	0.11 (1.89)	0.07 (1.25)
Second Quarter	—	—	—	-0.02 (-0.19)	0.05 (0.63)
Search Bonus Treatment					
First Quarter	—	—	—	0.05 (0.53)	0.00 (0.05)
Second Quarter	—	—	—	-0.12 (-0.93)	-0.04 (-0.30)
Wage Subsidy Agreement					
First Quarter	—	0.07 (1.27)	—	0.05 (0.78)	0.04 (0.66)
Second Quarter	—	-0.09 (-1.20)	—	-0.03 (-0.40)	-0.02 (-0.18)
Search Bonus Agreement					
First Quarter	—	—	0.29 (3.47)	0.19 (2.17)	0.19 (2.18)
Second Quarter	—	—	-0.10 (-0.91)	0.11 (0.86)	0.11 (0.88)
Log Likelihood	-8970	-8818	-9448	-27177	-26655
<i>n</i>	3845	3701	3865	11411	11411

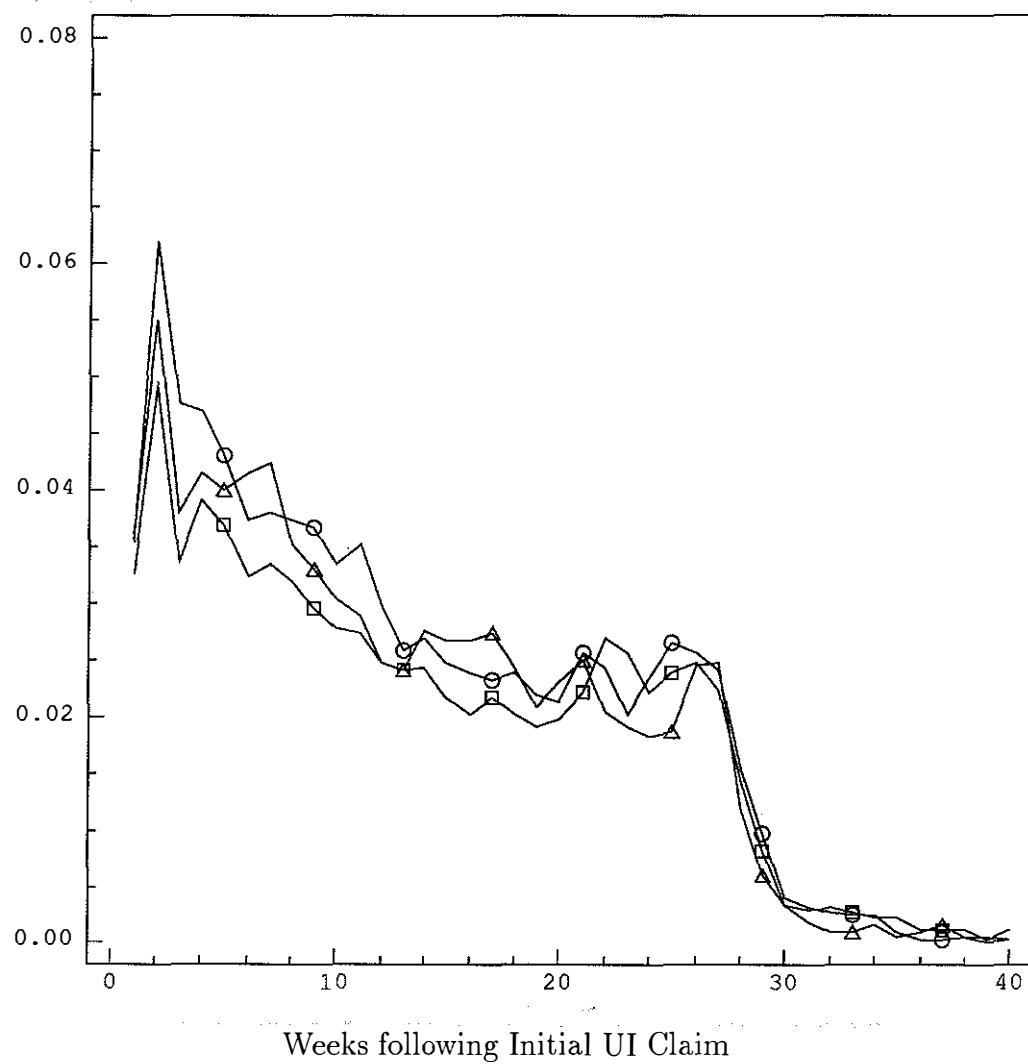
**t*-statistics in parentheses.

Table 8: Estimates of Average Weekly Wage*

	Uncorrected	Corrected
Constant	81.712 (7.438)	-41.82 (-5.029)
Wage Subsidy Treatment	32.619 (1.916)	43.389 (2.544)
Search Bonus Treatment	-7.189 (-0.266)	-3.006 (-0.111)
Wage Subsidy Agreement	-48.688 (-2.722)	-40.903 (-2.288)
Search Bonus Agreement	10.001 (0.370)	33.944 (1.247)
Base Weekly Wage	0.646 (24.670)	0.671 (25.383)
Selection Correction	—	111.7 (6.06)
R^2	0.106	0.112
n	5309	5309
Standard Error of Regression	348.3	347.2

* t -statistics in parentheses.

Figure 1: Percentage of Unemployed Workers Rehired



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